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ABSTRACT

Both as a method of measurement and as a theoretical hypothesis, syntactic maturity (SM) has profoundly affected thinking about verbal growth and ways to enhance that growth. On the positive side, SM is easy to use, requiring no elaborate training in grammar or statistics, and it is quantifiable providing an objective numerical index of significant structural characteristics. It is an explicit model for measuring growth in writing ability, correlating both to sentence combining and to general improvement in writing ability, and thus it has imposed on writing research a welcome added discipline, control, and degree of accountability. Syntactic maturity nonetheless creates some bothersome issues of methodology, such as how to obtain valid figures and how to interpret those figures. Examples of these difficulties include the inconsistencies in dealing with the extraneous, leftover material in sentences (garbles, fragments, ellipses, speaker tags, and the like), and the danger of reading too much into numbers, thus making the objectively measurable SM growth an end in itself. All of these issues, both the advantages and disadvantages, are generating pressure on writing researchers to collect more data and to make their results suitable for comparison. (RL)

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Measuring and Evaluating Syntactic Maturity: Some Questions of Methodology*

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Both as a method of measurement and as a theoretical hypothesis, syntactic maturity (SM)--or fluency--has profoundly affected our thinking about verbal growth and especially about ways to enhance that growth. Hunt's SM model (1965, 1970) has been applied to virtually every grade level, at least up to grade 13, as well as to language samples from adults beyond college age; to different ability or achievement groups (such as "superior" vs. "average", as well as so called "developmental" students); to spoken as well as written discourse; diachronically as well as synchronically, so to speak, to the study of long-term syntactic growth, as well as of age-specific stages of this growth; in the analysis of reading ability and of readability levels; in foreign language teaching and in the teaching of English as a foreign as well as a native language, and in the testing of the syntactic growth of youngsters in a score of other languages from Hawaii to Holland. SM has of course been instrumental in some significant research in writing, in the analysis both of writing samples with controlled content (such as in the use of the "aluminum passage")

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and of samples of so-called "free" writing in a variety of discursive modes. Until recently, SM had been associated in our minds solely with sentence combining as its supporting methodology, on the assumption that it is through practice in transformational sentence-maneuvering that students learn to consolidate sentences and to produce structures of greater depth and complexity--that is, more mature writing. Now there is evidence that SM can be significantly enhanced through practice in generative rhetoric as well (Faigley 1979). Perhaps reminding ourselves of this perspective is not without value: there is little doubt that the Huntian paradigm has inspired a new era of research in this field.

I would like to share with you some speculations--rather obvious ones--about why the SM idea has touched such responsive cords in the profession, especially among researchers in writing. But I want to temper these speculations by raising some bothersome issues of methodology, focusing especially on two kinds of problems: first, how to obtain valid figures for SM, and second, how to interpret those figures, once we have them.

What, then, has made SM such a useful principle?

(1) First of all, it is easy to use. To determine the SM values of a writing sample, you need no elaborate training in grammar or special expertise in statistics; for most purposes, all you need is to recognize clauses and to know simple division. Nor do you need training in linguistics, since most SM analysis deals with syntactic surface structure

and ignores the complexities of semantic representations as well as the more abstract transformations whose effects are not readily apparent in the "normal" syntactic pattern of sentences (such as certain nominalizations). In most studies, SM is represented as the simple arithmetic ratios of words-per-clause, words-per-T-unit, and clauses-per-T-unit. With minimal training, almost anyone can do a "T-unit analysis."

But the apparent simplicity of this procedure is misleading. Let me grab just one problem out of the many. No sooner do you begin to segment a paper into clauses and T-units than you find that some words and sequences of words do not neatly fall into such units. Trying to deal with extraneous leftover material such as garbles, fragments, ellipses, various ill-formed constructions, direct discourse, speaker tags, and the like, has been a headache for analysts since the earliest SM counts, as we can see from the inconsistencies especially in Mellon's (1969) and O'Hare's (1973) handling of such material. In the early studies, which most of us have used as models for our own work, it was standard procedure to discard fragments from the sample, except those that could be changed into complete sentences by the addition of a single word (presumably a verb). There was no doctoring of the data, no second-guessing what the writer had in mind: any material that did not belong to grammatically well-formed clauses was simply eliminated. But as T-unit counts are being applied to a wider range of samples increasingly removed both from the standard academic modes of discourse and from

the realm of the so-called "correct" standard dialect, the method--in its classical form--is running into trouble. Recently Hake and Williams (1979) have argued that all fragments should be retained and assigned to whatever sentence they logically connect to, regardless of the punctuation, thus providing a more accurate cognitive closure for the preceding T-unit--a closure that is presumably more consistent with the writer's semantic intentions. Here, clearly, the intent is to try to salvage the writer's thought, the content, at the expense of taking liberties with the form by inferring or reconstructing the logical links that the writer's misuse of punctuation has obscured. Here we are getting beyond the surface structure. Yet as we try to apply T-unit analysis to so-called "basic writing" of the sort illustrated in Mina Shaughnessy's Errors and Expectations, for example, we may not have a choice--lest after discarding all garbles, fragments, and malformed sentences from the sample, we have nothing left. Recently John Brereton and his colleagues from the Writing Development Center at the City University of New York circulated such samples among some seasoned T-unit analysts to see how they would cope with grammatically "deviant" data, and ended up with very inconsistent results (1979). The point is that as soon as we apply T-unit analysis--and try to determine SM--beyond a relatively narrow range of writing habits, we no longer have a standard counting procedure, and therefore often must resort to arbitrary criteria; the problem is that then we may go on comparing our results as

if we had all arrived at them the same way. A case in point is a study of freshman English samples by Ross (1971), in which mean clause length was computed after all "inaccurate and ineffective" clauses had been eliminated, and then the results were compared--and compared favorably--to Hunt's figures for skilled professional writers. Such a procedure is very hard to replicate. A nonsimplistic, nonarbitrary, and generalizable standard counting procedure is urgently needed.

(2) A second reason for the wide appeal of the SM measure is its quantifiability. The figures that we obtain through T-unit analysis provide an objective numerical index of at least one type of significant structural characteristic of a piece of discourse--or, better, of discourse produced by a group of individuals of a certain type. The point need not be amplified that we live in an age of quantification, where numbers and statistics permeate our lives and are highly valued as artifacts--or codes--that help us organize, classify, represent, prove, disprove, and even pass judgment on facts, objects, and events--in short, that help us to reduce complex phenomena to an objective level at which we can understand them, compare them, talk about them. Surely, SM figures serve some such purpose in our understanding of the complex process with which writing research is concerned. It is the availability of the SM measure that has made sentence combining, in Warren Combs' words, a "researchable methodology."

The main danger of quantification, especially of an aspect of writing ability, is that we easily read too much into the numbers, and come to

think of objectively measurable SM growth as an end in itself. Several experiments have shown that after a certain period of SC practice, students achieved a statistically significant increase in their T-unit or clause length. Recently Stewart (1978) reported, to cite one example, that in just six weeks he was able to raise his college freshmen's mean T-unit length (in free writing) by 4.25 words, to a posttest mean of over 18 words per T-unit. This is a whopping figure, a spectacular accomplishment--or so it seems: after all, by posttest time Stewart's freshmen came close to the 20-word average of Hunt's skilled professional writers; and indeed on the aluminum test, Stewart's students, with their T-unit length of 16.13 words, far surpassed Hunt's so-called skilled adults (14.24). But there's a hitch. On the one hand, behind this seemingly inferior figure for skilled adults we find excellent, superior writers, whose superiority we ultimately judge subjectively, only by how their words affect us, the readers--and of course by how they affected the editors of Harper's Magazine and the Atlantic, who thought those words good enough to publish. Stewart's syntactically mature college students, on the other hand, did not end up writing any better at the end of their crash course: the quality, the "readability" of their writing showed no improvement. What, then, do such spectacular quantitative figures mean? In what sense do they mean "growth"? What value can we attach to them--and to the pedagogical procedures that generated those figures--if we take them to be the end of the road? Such results pose obvious problems of interpretation.

(3) Third, the concept of SM has, nevertheless, provided researchers with an explicit model for measuring one type of growth--or apparent growth. I am using the term "model" in a twofold sense here. On the one hand, Hunt's index in effect defines degrees of "maturity" as discrete points on a scale of ascending numbers, and enables us to conceive of and represent syntactic growth as an upward movement from any given point on this scale to a higher point. On the other hand, the index also provides a model in the sense of a goal to be achieved, a target to be reached, as a result of a certain kind of sentence-oriented practice. Getting your students to raise their T-unit or clause length from point X to point Y on this scale gives you tangible evidence that your teaching method works--and, by comparison, perhaps works better than another method--and that you have moved your students a bit higher up on this magic ladder toward the top where, with their average T-unit length of over 20 words, the cream of the crop of writers is to be found. Grading student papers week after week, year after year, does not lend the teacher such a psychologically reassuring perspective.

Yet, quite apart from the need to validate or make more meaningful the increases in the SM factors by other, perhaps more qualitative checks, the question looms large just how much of such increases really represents growth--that is, a cognitive enrichment over which we have no conscious control--and how much is a more transitory, half-conscious adjustment of style in response to intensive sentence-oriented instruction. This

question has been recently raised by John Mellon (1979) in a highly significant theoretical statement presented at the Miami Conference on Sentence Combining in October 1978. Mellon claims that only an expansion and elaboration of restrictive modifiers embedded in dominant nominals can be considered growth; large increases in "nonrestrictive surface-structure combinings," he says, "do not represent stable changes of any kind" but are rather "optional performances of thought" or "secondary statements" that, certainly by college age, after the age of decentering, can be freely produced as a matter of choice, through a voluntary, conscious, and controllable process. The implications of this distinction are exciting. One would like to go back and reanalyze the large bodies of data which have yielded all the published figures for syntactic maturity growth. In an informal way, we have reanaly: d the ten papers in our Miami study (Daiker et al. 1978; Morenberg et al. 1978) that showed the largest pre-post gains in clause length, by recounting separately the restrictive and the nonrestrictive modifiers of the two major dominant nominals, subjects and objects. In the almost 5 1/2 word pre-post gain in the size of such modifiers per T-unit in this sample, only about a 1.5 word gain occurred in restrictive modifiers and a gain of nearly 4 words in nonrestrictive modifiers. If this is even remotely representative of what happened in the rest of the 600 papers, then indeed the amount of "growth" in Mellon's sense is somewhat dwarfed by the preponderance of so-called secondary statements, whose main value or function is

presumably stylistic. But I am not happy with this interpretation and its implications, because all but one of these ten writers--all of whom, by the way, happened to be from the sentence combining sections of the program--also registered gains in the quality of their writing, as judged by innocent, independent judges. If these students did not "grow" much in their syntactic maturity--as measured in terms of restrictive modifiers--they certainly grew in their maturity as writers. Perhaps these two types of "maturity" are not only not at all co-terminous, but are not even directly related. This speculation would jibe well with the results of our regression tests performed on the entire corpus, which showed that the three standard syntactic maturity measures together predicted less than 4 percent of the holistic rating. Faigley (1979) came to much the same conclusion. Yet I would bet that, in this limited sample, those 5-6 extra words in the students' nominal modifiers--even though mostly assigned to nonrestrictives--had a lot to do with making the papers qualitatively better. Those words may have brought extra detail into the essays--the stuff that can make writing vivid, concrete, and interesting. They may have helped widen the writers' syntactic attention span or short-term memory and thus made them more perceptive of semantic closures and semantic links. Perhaps "growth" in Mellon's sense does peter out by college age, and the effects of sentence combining begin to show up in alternate ways, such as syntactic variety and other such measures of stylistic finesse. But they do show up. Let me mention

in this connection that Morenberg, Daiker, and I have reanalyzed the entire corpus from our 1976 study for syntactic variety, that is, for variation in T-unit length. Critics have argued that sentence-combining practice leads to lengthy, complex, and unwieldy sentences. We compared sentence-combining students to control students. A one-way analysis of variance showed that after one semester, the sentence-combining students varied their sentence length significantly more (beyond $p < .001$) than did control students. By a process that we do not quite understand, the sentence-combining students appear to have translated their increased SM into a stylistic quality.

(4) This brings me to my next reason--perhaps the most obvious one--for the appeal of the SM paradigm: the correlations found between SM gains presumably induced by sentence combining exercises and a demonstrable improvement in the overall quality of student writing. O'Hare's discovery of this correlation (1973) made sentence combining history, and may yet make composition history. It will be paralleled perhaps only by a new breakthrough that will enable us to understand how and why this correlation exists--an insight that at this point remains hopelessly elusive.

(5) Finally, let me cite one more reason why we have embraced the SM model: it has imposed on our research a welcome added discipline, a control, a degree of accountability that are difficult to establish where measurement remains purely subjective, as in so

much of the composition business. Because SM is computed through a standard mode of measurement and because it is expressed in numbers, the values obtained by one researcher can be easily tested--and confirmed or disconfirmed--by another. What has made SM such a powerful concept above all--I think you will agree--is the consistent, repeated confirmation of its norms. It now appears to inhere in the graphic-verbal competence of, say, an ideal, average fourth grader to write T-units with a mean length of about 8 1/2 words, or of twelfth graders to write T-units about 14 1/2 words long. Perhaps these figures do reflect a developmental timetable built into us; in any case, the norms, it seems, are absolute, and as such, they allow us--indeed force us--to put our experimental SM results in their perspective. Variant results alert us to look for experimental conditions that might explain the variance. Such scrutiny might show a study to be faulty and thus its results questionable, but it can also lead to new insights, even to a refinement of the norms themselves. In any event, the SM paradigm has generated a pressure to make our research results suitable for comparison: it has given us a common language that enables us to talk to one another--a shared underlying framework that admits divergent findings but also gives us a vocabulary in terms of which to try to account for them.

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